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## ANOTHER ASPECT OF THE SPECIES QUESTION

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THE AMERICAN NATURALIST for April last (Vol. XLII, No. 496, pp. 217-281) contains a report of the symposium held by the Botanical Society of America at its recent Chicago meeting on "Aspects of the Species Question." This series of eight papers should be of great interest to zoologists as well as to botanists, since it brings so strongly into relief the differences in view-point held by leading taxonomers in two widely different fields of research.

The wide divergence of views here shown in relation to the "species concept" is no less surprising than are the measures urged by some of the speakers for the reduction of "taxonomic anarchy" in botany to some degree of reasonableness. The ecologic botanist needs some method of designating the minor variations which are so important to him in his researches, but from his point of view the question "What is a species?" can be decided only by experimental research. In the meantime we may go on naming forms without essential modification of present methods. From another point of view, the naming of new forms is to be repressed with the utmost rigor, even to the establishment of a "taxonomic censorship" upon "species-making." While most of the speakers appear to agree that there are no species in the Linnean sense, and that species are merely mental concepts, like genera and the higher taxonomic groups, there is a tendency to hark back to the Linnean concept, and to accept the Linnean standard as far preferable to the minute discriminations of modern taxonomers. From other points of view there may be physiological species as well as morphological species; there may be species that

are fixed and immutable, others that yield readily to the forces of environment. That so-called species originate by different methods is evident, thanks largely to modern botanical research. The whole outcome of this symposium, as it affects nomenclature, is naturally indecisive, but contributes a few reasonable suggestions for its improvement. It is, however, a brilliant exposé of conflicting interests and conceptions.

The state of unrest, and the dissatisfaction with the *status quo* here disclosed, are not confined to the ranks of the botanists; the evils here arraigned have long agitated zoologists as well. But the organisms botanists have to deal with are so different from animals, at least the higher forms of animal life, in methods of reproduction, manner of growth, and response to environment, that the species problem in botany is a far more complex question to deal with than it is, for example, in the higher vertebrates, with which alone the writer claims familiarity.

Where to leave off naming "forms" is, after all, the main point at issue, and the next, how best to express their relationships as regards origin and degree of affinity. Utility should be, of course, the criterion in both cases, but especially in the former. How to designate the minor forms, and to what extent they may be profitably provided with names are questions that agitated zoologists apparently long before they became serious problems with botanists. Zoologists, certain of them at least, long since reached what seemed to them a satisfactory solution of the species question by recognition of the fact that "species," like genera and the higher groups, have no real existence, but are merely man-made concepts, purely arbitrary and conventional, devised for convenience in recording our knowledge of organic nature. More than thirty years ago this concept found practical recognition among American ornithologists and mammalogists, and from this hotbed of heterodoxy the infection rapidly spread until this point of view became the working basis

of their coworkers throughout the world. As early as 1884 this postulate was incorporated in the A. O. U. Code of Nomenclature (first published in 1886), as a reason for the adoption of trinomials, in the following words:

Recognition of the scientific fact, that a "species," so-called, is not a fixed and special creation, as long supposed, but simply a group of the same intrinsic character as that called a genus, though usually less extensive, and always of a lower taxonomic rank, has done more than any other single thing to advance the science of zoology; for the whole theory of evolution turns, as it were, upon this point.

Respecting the meaning and function of the trinomial system, it is said in the same connection:

Trinomials are not necessarily to be used for those slightly distinct and scarcely stable forms which zoologists are in the habit of calling "varieties"; still less for sports, hybrids, artificial breeds, and the like; nor indeed to signalize some grade or degree of difference which it may be desired to note by name, but which is not deemed worthy of a specific designation. The system proceeds upon a sound scientific principle, underlying one of the most important zoological problems of the day,—no less a problem than that of the variation of animals under physical conditions of environment, and thus of the origin of species itself. The system is also intimately connected with the whole subject of the geographical distribution of animals; it being found, as a matter of experience, that the trinomial system is particularly pertinent and applicable to those geographical "subspecies," "races" or "varieties" which have become recognizable as such through their modification according to latitude, longitude, elevation, temperature, humidity, and other climatic conditions. It is obvious, therefore, that the kind or quality, not the degree or quantity, of difference of one organism from another determines its fitness to be named trinomially rather than binomially. In a word, *intergradation* is the touchstone of trinomialism.

A small amount of difference, if constant, was considered "specific," in a proper sense, while a large amount of difference, if found to lessen and disappear when specimens from contiguous faunal areas were compared, was considered as not specific.<sup>1</sup>

While these distinctions and principles have been found to work well in vertebrates, and have become to a

<sup>1</sup> Cf. A. O. U. Code of Nomenclature and Check-list of North American Birds, 1st ed., 1886, pp. 27, 31, and context.

large extent the working basis of zoologists, it is evident that they must necessarily fail to meet the needs of botanists, where the minor differentiations are of such a different character, and where so many and such diverse conditions affect differentiation within groups formerly recognized as species. Botanical nomenclature evidently must remain a makeshift, satisfactory to no one, until the ecological, physiological and experimental botanists have come to a better understanding of the causes and relations of morphological and physiological differences among closely related forms.

The introduction of trinomial nomenclature into zoology, while satisfactory for the designation of intergrading geographic forms, left still unsettled the old question of how far it is profitable to recognize in nomenclature the minor morphologic differentiations. In the cases of mammals and birds it was found necessary to reduce many so-called species to subspecies, and also to add, as material increased, many new forms under trinomial designations. These were, of course, intergrading forms, and hence arose the serious question of where to stop bestowing names upon slight morphologic variations, restricted to limited and not very clearly defined physiographic areas. As no hard and fast rule can be formulated, the treatment of such cases becomes largely a matter of temperament, experience and good judgment, in other words, the fitness of the taxonomer for his work. As is well known, the American Ornithologists Union has a permanent committee on the nomenclature and classification of North American birds, whose function it is to consider all proposed changes in nomenclature and all proposed additions to the list of North American birds. This committee, consisting of seven members, is reappointed annually, and is subject to change in personnel. Its reports are published from time to time in the form of supplements to the A. O. Check-List of North American Birds, in which are given not only the accepted changes of nomenclature and the additions to the list, but also

briefly the reasons for not accepting certain of the proposed nomenclatural changes and additions. While, its authority is, of course, not final, except as regards the A. O. U. Check-List, it can throw the weight of its influence against innovations that in its judgment are detrimental to the best interests of the science. It undoubtedly acts as a powerful deterrent upon ill-considered work, and the general acceptance of its decisions by amateurs and laymen tends to the conservation, in this country at least, of a standard and uniform nomenclature for North American ornithology. It is a mild form of censorship—far less radical and drastic than that proposed by some of the botanists at their recent Chicago meeting.

As already said, the conditions in regard to the nomenclature of minor forms which confront botanists are very different from those met with in the study of terrestrial vertebrates. There is no group in mammals, birds or reptiles comparable in respect to the number, character, and association of its minor forms with *Crataegus*, *Rubus*, *Aster*, *Sisyrinchium* and numerous other botanical genera, where several recognizable morphologic forms occur in intimate geographic association. In the vertebrate classes named, the forms recognized in nomenclature that occupy the same area are always sharply separated; there is never a question of specific or even subspecific identity, except in the case of migratory birds, where several different geographic subspecies may be found associated in migration, or during the non-breeding season, in districts remote from their respective breeding ranges. Questions of identification arise, not between locally associated forms, but in reference to whether individuals from intermediate areas are referable to one or the other of the several subspecies that occur typically only in adjoining physiographic (faunal) areas. An apparent exception is afforded by the red crossbills (*Loxia curvirostra* group), where a wide range of so-called individual variation is met with, affecting the general size,

to some extent the coloration, and especially the relative size and massiveness of the bill. The extremes of variation in the latter are enormous for the size of the bird. As such individuals occur, however, more or less generally throughout the exceptionally wide (almost continental) range of the species, and are thus not characteristic of any particular locality or region, and also are everywhere associated with birds of intermediate characters, these divergent "forms" are not considered by ornithologists as entitled to nomenclatural recognition. Some years since a large-sized, large-billed, very red form was described as a subspecies under the name *Loxia curvirostra bendirei*, based on specimens from eastern Oregon. It was long held in abeyance, however, by the A. O. U. Committee, and finally rejected on the ground that no definite or distinctive geographic range could be assigned to it.

This case is cited as an illustration of the wide differences in conditions that obtain in botany and zoology. If the crossbills grew fixed to the soil like plants, and their ecology was equally open to investigation, some cause might be assigned for the development, respectively, of the large-billed and small-billed forms. At present all we know is that the common red crossbill is an exceedingly erratic species, nesting apparently with equal frequency in mid-winter or in summer, and migrating with extreme irregularity as to season and the regions visited, and characterized by an exceptionally wide range of morphologic variation. The variation in the general size of the individual, and in the size of the bill and in coloration may result from different food habits, and the lack of any sharply defined lines of morphologic separation may be due to frequent or habitual interbreeding of the different morphologic strains. The peculiar life habits and general economy of the species seem to hopelessly remove it from the field of experimental research, at least for the present.

Considering utility as the test, the trinomial system

fairly meets requirements in the case of the higher vertebrates; it seems to apply less satisfactorily in the lower animal phyla and in botany. It is admittedly preferable, however, in botany, in a large number of cases, to the binomial method, even where the variation is not geographic, since it denotes close affinity and supposed ancestry. The suggestion of Clements (*l. c.*, pp. 262-264) that, where new names are required, the third term be chosen with reference to the nature and origin of the variant, seems to offer distinct advantages.

It seems probable that the "species concept" in botany, as in zoology, can be settled only by conventional agreement; and the many interests and the diverse fields of research to be considered and harmonized, seem to relegate this desirable event to the remote future. The present diversity of views respecting the desirability and the manner of designating minor forms does not forecast a speedy agreement on even this important phase of the subject. If ever agreement is reached it obviously must come about through concert of action and mutual concessions. No radical scheme of censorship should be seriously entertained; any attempt to restrain the exercise of individual judgment will be strenuously opposed. Each man's work must stand or fall on its merits, in matters of taxonomy as in other fields of scientific research; the insistence on Latin diagnoses is a step in the direction of medieval intolerance, and apparently had its origin in personal jealousies rather than in a desire to advance the true interests of science. The ability to write in a dead language is no measure of a man's ability as a taxonomer, nor of his scientific sanity, nor of his general level-headedness as a naturalist. As said by one of the participants of the symposium:

"What constitute sufficient morphological characters [for a species or for a minor form] must be left to the individual judgment."

The proposition to recognize by name species only and to designate minor forms—the subspecies or "races"—



numerically seems to have met with some degree of approval; which is natural, since, at first sight, it seems to present some advantages. But "*Quercus alba* race 2," or "*Draba verna*, race 104," can mean little unless the authority for the race be added as "race 2 Brown," or "race 104 Smith"; since different authors may be at work almost simultaneously upon the same group, so that race 2 or race 10 of A may not be the race thus designated by B, who at the time was ignorant of what A had done. There would be under this new method the same opportunity as at present for synonymy to play its usual rôle, the same necessity for the strict enforcement of the rule of priority. It is thus hard to see how the conditions would be improved by the adoption of a bare numeral in place of a name, which generally has some useful suggestiveness. If the authority for the race must be added to secure absolute definition, as would certainly be necessary in many instances, very little would be gained in point of brevity, and much lost in the way of aid to the memory. Should such a system be adopted and be made retroactive, chaos would hold place for an indefinite period, and the familiar verbal designations of the past, with their historic suggestiveness, would eventually share the fate of the designations of the herbalists of the past centuries. But there is probably little cause for alarm, since any serious attempt to introduce numerical designations in place of trinomials must, in all probability, demonstrate their impracticability. The proposition is more novel than practical, a protest against burdensome conditions rather than a panacea.

Another point brought out in this series of papers, though of perhaps no great importance, invites comment. It is stated (*l. c.*, p. 246):

"If I understand aright, the ornithologists, at least some of them, are willing to assert that species may differ by characters which can not be described by words [laughter]; that is to say, you will know the species when you see it, but you can not describe it in any language by which another person may recognize it. I think that this is not a

joke, but that it is really stated in earnest; because I read it in *Science*. [Laughter.] But I fancy botanists have not gone quite that far."

The author of this statement, which seems to have excited so much merriment, was simply mistaken. It would have had more basis if he had said subspecies, or "forms," instead of species, but even then would have been misleading and unfair. There are plenty of instances, among both birds and mammals, where local forms of a species are perfectly recognizable by any one as morphologically different when directly compared, through very obvious differences in coloration. But shades of color perfectly apparent to the eye are hard to characterize in language, from the fact that the color terms in current use are vague and uncertain, so that different persons may habitually describe the same tones of color in different terms, and use the same phraseology for quite different tints. It is for this reason that direct comparison of specimens is so often necessary; not that the forms themselves are so difficult to distinguish. The statement that "botanists have not gone quite that far" in describing "forms" is hardly borne out by the confession of another participant in this symposium, who says (*l. c.*, p. 240):

"The general result of these attempts to dissect nature has been embarrassing because when a subsequent student takes up the group he is wholly unable to determine from any descriptions that can be written where any given individual would have been grouped by the previous author, unless he has access to the actual specimens which the previous author studied," etc.